ever, the question is the somewhat simpler one of determining the conditions under which a bird can gain elevation without expending energy, velocities relative to the earth may, of course, be ignored.

There is, as I now see, a great advantage in making the simpler investigation first: for, as Dr. Kimball has clearly shown, as soon as we recognize the fact that the bird's motion relative to the medium depends only on their relative velocity, it becomes clear that gain of elevation, and consequently the whole phenomenon of soaring, is impossible in a uniform horizontal wind.

It follows that there was an error in my theory of soaring. Mr. Gilbert thinks it due in part to my assuming it to be possible for a bird to glide in a wind moving faster than itself, with its head to leeward; but I see no reason why birds should not accomplish this fact, and am satisfied that I have often seen them do it. He also holds that my bird, "in passing from a negative velocity relative to the air, to a positive velocity relative to the air, must pass through the phase of no velocity relative to the air, in which he is practically helpless." But I was dealing with the bird's component velocity in the line of the wind's motion; and he might always have a velocity relative to the air, though its component in that line might be zero. The error which I made was in assuming, that, under the conditions of flight to which I subjected my bird, the turn to leeward was possible. From the way in which I made him fly, it is clear that the resultant force exerted on him, at every point of his supposed path, must be upward and to leeward. That being the case, the turn to leeward could not be accomplished, and consequently the path he was supposed to describe was an impossible path.

I feel that I must apologize to those of your readers who may have followed me in what may fitly be called "a wild-goose chase."

J. G. MACGREGOR.

Dalhousie College, Halifax, N.S., March 8.

"Shall We Teach Geology?"

IN Professor Winchell's remarks on my review of his recent work, there are only two points that call for reply. First, as to the study of history, which, according to him, trains no faculty but verbal memory. He now says that his "criticisms on history contemplate it as a study urged upon children in the early stages of education," and that in the colleges it is pursued in a better way. But, even if imperfectly taught, history trains far more important faculties than verbal memory. It exercises the intellect generally quite as much as geology does, and it also calls into play the moral judgment and the sympathies, which geology does not. To Professor Winchell the old red sandstone may be a more important topic of study than the Roman Empire, and the plesiosaurus a more interesting object of contemplation than Washington or Columbus; but to the mass of men this is not so. As to the time that Professor Winchell would have spent on geology, I may have misapprehended his meaning; and, if so, I am glad to be corrected I haven't his book by me at present; but, if I remember rightly, he says that the study ought to be taken up in the primary schools, and *continued through the various grades*, which I understood to mean that the subject should be studied more or less every year. He now says that he only wants it taken up several times at intervals, and not pursued continuously, which is more moderate. I do not see, however, how even so much study of geology is possible; because, not to speak of languages and literature, there are many sciences of greater importance than geology, which ought, therefore, to be studied first. Such are arithmetic and geometry, geography, physics, human physiology, psychology, ethics, civil polity, and history; and I do not see how even all of these can be taught in the public schools. If these views are correct, geology can be nothing but an optional study in the high schools and colleges, while in the lower schools it can have no proper place.

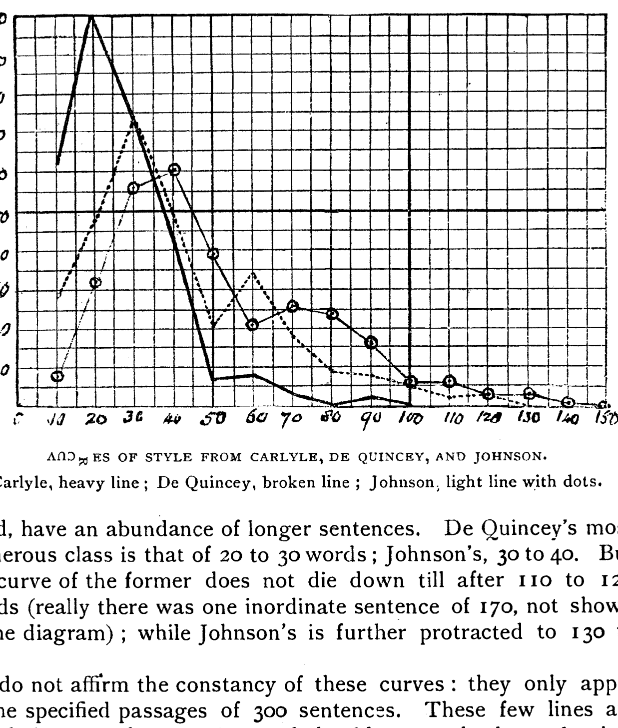
THE REVIEWER.

Curves of Literary Style.

IN the interesting researches on this subject by Professor Mendenhall described in your journal in 1887, words were classified according to the number of letters in them, and curves made accordingly. As he pointed out, there are many ways in which the principle of his method may be applied; and I have lately thought some instructive results might be obtained from examining sentences with regard to length, as measured by the number of words.

Length of sentences is a matter in which pronounced styles differ greatly. Doubtless this is associated with psychological peculiarities which it might be instructive to inquire into. The mental machine (so to speak) which, for example, turns out the long parenthetical sentences of Gladstone, must be very different in design from that which yields the simple and direct utterances of John Bright.

I have made an examination of 300 sentences in each of the following works: Carlyle's "French Revolution," De Quincey's "Confessions," and Johnson's "Rambler." The number of words in each sentence was counted, and the sentences grouped accordingly. Then the sentences with words up to 10 were added together, those with words from 10 to 20, from 20 to 30, and so on. The accompanying curves were then obtained from these data. Let it be clearly understood what they mean. The plain line curve (for Carlyle) means that in the 300 sentences of the passage selected there were 62 containing words varying in number up to 10, while 100 had from 10 to 20, and so on. The result is roughly as we might expect: short sentences form the bulk of the Carlyle passage, his maximum being in the class 10 to 20, and sentences of more than 50 words are comparatively few. There are none beyond 100. De Quincey and Johnson, on the other



ANALYSES OF STYLE FROM CARLYLE, DE QUINCEY, AND JOHNSON.
Carlyle, heavy line; De Quincey, broken line; Johnson, light line with dots.

hand, have an abundance of longer sentences. De Quincey's most numerous class is that of 20 to 30 words; Johnson's, 30 to 40. But the curve of the former does not die down till after 110 to 120 words (really there was one inordinate sentence of 170, not shown in the diagram); while Johnson's is further protracted to 130 to 140.

I do not affirm the constancy of these curves: they only apply to the specified passages of 300 sentences. These few lines are merely by way of suggestion, and should any reader have the time and patience to pursue the inquiry further, he might, I think, find his labors not without some useful results.

It might be useful to see in what degree these curves approximate to constancy, or come short of it. One would like to know better than we do at present, how far the method, in any of its forms, is reliable or helpful in settling disputed questions of authorship, or in tracing anonymous literature to its source.

I would suggest an examination of the words used by speakers or writers as likely to be instructive.

A. B. M.

London, March 7.

Wind-Velocity and Wind-Pressure.

FROM time to time there have appeared discussions of these questions, so important to the practical engineer. It seems probable that the first of these, as far as relates to the relation between wind-movement and the travel of the cups of Robinson's anemometer, is soon to be definitely settled by indubitable experiments.

Professor Robinson first considered that the cups moved with one-third the wind-velocity, but this has been repeatedly called in question. In later times the more common method of investigation has been by whirling the anemometer on arms from 11 to 35 feet in length. It would seem as though arms of 11 feet could hardly give satisfactory results.

In discussions of this relation, the utmost confusion has arisen by wrongly considering the so-called "anemometer factor," and by making the same an entirely different quantity, and one from which it was supposed a "friction constant" had been separated. The statement that anemometers used in this country give 20 per cent too great wind-movement has been based on this misconception. Let $x =$ "anemometer factor," $w =$ wind-movement, and $v =$ travel of the cups: we have,

$$x = \frac{w}{v} \qquad (1)$$

Let $a =$ "friction constant," and $b =$ another constant: we have,

$$w = a + bv \; (2)$$

Substituting the value of v in (1), we have,

$$x = \frac{bw}{w - a} \; (3)$$

In experiments at St. Petersburg it was found that an anemometer with 6.72-inch arms and 4-inch cups, the same as used in this country, had $b = 2.47$, and $a =$ about 2 miles per hour. Assuming w at various velocities (5, 10, 15, 20, and 25), we obtain from (3), for x, 4.12, 3.09, 2.85, 2.74, and 2.68 respectively.

We see that even these earlier investigations show our anemometer (with factor 3) almost exactly correct for velocities from 10 to 15 miles per hour, while at less velocities it gives too little wind, and only about 12 per cent too much at 25 miles.

The wind records of this country had been so often called in question, the chief signal-officer finally made provision for an investigation of the question. The results in full will shortly be published. For our present purpose it will suffice to give the approximate results with our own anemometer, described above: with w at 5, 10, 15, 20, and 25, we obtain for x, 3.30, 3.11, 3.05, 2.98, and 2.89 respectively. These are very satisfactory, and show, that, except for high or low winds, the records are entirely correct.

It is rather singular that investigations have recently been made in England with a whirling arm of 29 feet, almost the same as that used in this country (28 feet). Unfortunately these experiments were made in the open air, and with a natural wind often 4 miles per hour. These currents vitiated all the results for velocities less than 30 miles per hour: in some cases the error amounted to 35 per cent. The helicoidal anemometer which was tested had a vane attached to keep it in the wind. It is of the same nature as the "air-meter," long since discarded for wind measurement, and only used for straight-line currents in mines or elsewhere. Fortunately in these experiments there was one day when it was nearly calm, and the results for that day do not differ from others made in a closed court. For velocities less than 25 miles per hour, these results are entirely unreliable and misleading, in the present state of our knowledge of the problem. An extended discussion of this question will be found in the *American Meteorological Journal* for March.

While much time has been expended on the above problem, yet much more has been spent in determining the relation between the velocity and pressure of the wind. This problem is by far the more difficult to solve, and to practical engineers the more important of the two. One thing is very gratifying, and that is that the investigations and practice so far have been almost entirely on the safe side; and the wonder is that buildings have blown down at all, at least if engineers have ever allowed the commonly accepted figures to enter their computations. It is probable that in most cases engineers have assured themselves of a factor of safety far beyond any thing that any experiments have indicated. How is it that if, as some claim, the usual deductions have indicated three times too great pressure of the wind, any building has ever blown down? If we examine the matter, however, we shall find that most of the theoretical discussions, when separated from well-conducted investigations, will lead and have led far astray. One of the most astonishing misapplications has been of Hagen's experiments, made with plates from 2 to 6 inches square at velocities from 1 to 4 miles per hour, to the side of a house 400 inches square, and with velocities of 60 or 70 miles per hour. But this is not all. Even Hagen's experiments are repudiated by those very persons who make this application, for the reason that they give an increasing pressure as the plate grows larger; so that with a house 400 inches square the pressure, according to Hagen's formula, would be seven times as great per square foot as on a plate 4 inches square. Certainly it would be very unscientific to discard the application of a formula where it does not seem satisfactory, and then apply the computation at another portion of the formula to that portion where we have discarded the same formula.

The best experiments with low velocities show no increase in pressure per square foot for plates from 4 to 24 inches square; and when plates have been exposed to the free wind, or at very high velocities, the result has shown

$$p = .005\, sw^2,$$

in which $p =$ pressure, $s =$ surface in square feet, and $w =$ velocity of wind in miles per hour. The recent English experiments were with a plate 6 inches square; and, even if they were not vitiated by untoward causes, it would be utterly impossible to reason from them to what the pressure would be on a surface four thousand times as great. H. A. HAZEN.

Washington, March 18.

Queries.

44. EQUILIBRIUM. — In the account of his travels in the Colonies, the Marquis de Chastellux relates, that while at Albany, Jan. 1, 1782, he was surprised at the noise and racket with which the new year was ushered in; young folks, servants, and even negroes going from tavern to tavern, singing, and asking for drink. New Year's morning he took leave of Gen. Clinton, and adds, "I met nothing but drunken people in the streets, but what astonished me most was to see them not only *walk*, but *run upon the ice, without falling or making a false step, whilst it was with the utmost difficulty I kept upon my legs*" (*Travels in North America*, 1780–82, London, 1787, p. 441). Here is the best of evidence (for the marquis related only that which he saw; and his narrative, as well as being the most interesting "*private*" view of our country at that critical period, is also the most trustworthy), asserting that in some way a drunken person, or one not having to the fullest degree what we may call self-control, has a decided advantage over his supposed clearer-headed brother, who has refrained from the "flowing bowl." Is this actually the case, or is the advantage more apparent than real? Most of us have at some time noticed the truly wonderful balancings of a drunken person when in proximity to a curb or flight of stairs, and have commented thereon that a person conscious of the position could not imitate these contortions without danger to life and limb. Does extreme mental alertness, then, act as a detriment, while a blunted sensibility is an advantage to the person so conditioned? If so, the question becomes an important one, and not confined to conditions of self-imposed disability. We may need to know definitely at certain critical periods whether, in order to accomplish a given object, it is better that we should be partially blindfolded than that we should see and know all.

A. M.

Indianapolis, Ind., March 13.

Answers.

42. LOOKING TO THE LEFT. — In answer to Query 42, permit me to suggest that seats on the right as one enters a play-house are preferred, because the action on the stage is to the observer's front and left. Troopers, choruses, and principals come on the stage from the left side; and dialogue, combat, and chief business generally occur in the corner back and to the left; while the mob, as in Cæsar, and Spartacus the Gladiator, fills in the right. This is the rule in our experience, modified in some cases by the limitations imposed by the building. Again, how will "42" account for the fact that abroad, confined perhaps to England only, if you turn to the left you are right, while if you turn to the right you are wrong? L. E. J.